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Movements in Julian century $\div 10^6$:—

In geocentric elongation in longitude	1681″·6
In geocentric latitude	165·3

Stiklastad 11° 35′ E; 63° 38′·8 geocentric latitude; $1-\rho = 0{\cdot}00268$

Parallax in longitude +210″·7; in latitude −3122″·8

Movements in Julian century $\div 10^6$ as seen from Stiklastad:—

In elongation in longitude	1480″·3
In latitude	44·5

Hence if $0{\cdot}09\Delta F$, ΔD are the corrections required by the latitude and difference of longitudes, then the latitude at apparent conjunction in longitude is

$$-8'' + 0{\cdot}09\Delta F - 0{\cdot}03\Delta D$$

The difference of apparent semi-diameters is about 5″.

Note on the Approaching Return of Halley's Comet.

By A. C. D. Crommelin.

It is well known that Dr A. J. Ångström published in 1862 a paper entitled "Sur deux inégalités d'une grandeur remarquable dans les apparitions de la comète de Halley" (Actes de la Société royale des Sciences d'Upsal, Sér. III., t. iv.). In this paper he discusses the observed perihelion passages, as determined by Dr Hind, from B.C. 11 to A.D. 1835, and deduces the mean period of the comet, 76·93 years, this period being affected by two large inequalities of amplitudes 1·5 years, 2·3 years, periods 2650 years, 782 years. He has found theoretical arguments which will satisfy these periods, viz. 13 ☄ - 2♃, ♃ + ♄ − 9 ☄, where the symbols denote the mean annual motions of comet, Jupiter, Saturn. The amplitudes have been obtained by observation, and in no case does the error in the formula as compared with the observed time of perihelion exceed 1 year; in most cases it is less than half a year. Dr Ångström does not claim that these two are the only inequalities, merely that they are the most important ones.

I do not think it is so generally known that the time of the next perihelion passage, as deduced from Dr Ångström's curve, is altogether different from that published by Count de Pontécoulant. I deduce from the curve 1913·08 for the time of the next passage, whereas that given by Count de Pontécoulant is 1910·37, a discordance of 2·7 years.

We are not, of course, justified in assuming that M. Pontécoulant's

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result is erroneous, solely because it differs from Dr Ångström's curve. For exact numerical computation is entitled to far more weight than an empirical method. But it is difficult not to feel some slight uneasiness about the matter, when we consider the extreme length and intricacy of the calculations, and the possibility that some important numerical error may have escaped detection. It is surely a *desideratum* that the perturbations should be independently computed, and I hope that some mathematicians may be actually engaged in the work; if not, there is still time for someone with the necessary leisure and ability to undertake it. Before the 1835 return there were at least five independent computations of the orbit—those of Damoiseau, Pontécoulant, Lehmann, Rosenberger and Lubbock; and it is difficult to understand why an equal amount of interest is not shown in the approaching return. Pontécoulant's result was published in 1864, and doubtless he regarded it as certain that there would be numerous investigations when the time drew nearer, so that he may well have given somewhat less attention to the next return than he gave to that of 1835. This is borne out by the fact that there are certainly some slips or misprints (not having seen his manuscript, I cannot say which) in his paper as printed in *Comptes Rendus de l'Académie*, tome lviii. Thus on p. 826 he gives T as 1910 May 24·37, Paris civil time (I have verified that this is the correct value from the figures and formula that he quotes), but on p. 828 T is given as May 16·95; this is merely an error of copying, and is corrected in a note in a subsequent number of *Comptes Rendus* (p. 915 of the same volume). The *Connaissance des Temps*, however, quotes the latter result converted into astronomical reckoning; while Dr Hind (*Encyc. Brit.*, 9th edit., vol. vi. p. 193) quotes the former, which is unquestionably the right one to take. Another case is that of the perturbation in the eccentricity. This is made up of three terms due to Jupiter, Saturn, and Uranus, the separate terms and their sum being given as follows on p. 827 :—

Planètes.	Altération de l'excentricité $\int de$.
♃	− ·00529752
♄	− ·001104850
♅	+ ·000040625
Total,	− ·005655833

It is at once obvious that the total does not agree with the separate terms as printed, nor am I able to suggest any conjectural emendation which will bring them into harmony.

This value of the total is used in deducing the eccentricity as given in the elements for 1910, and it is noteworthy that both the eccentricity and perihelion distance for that epoch show a striking discordance from those in earlier apparitions, as this table shows.

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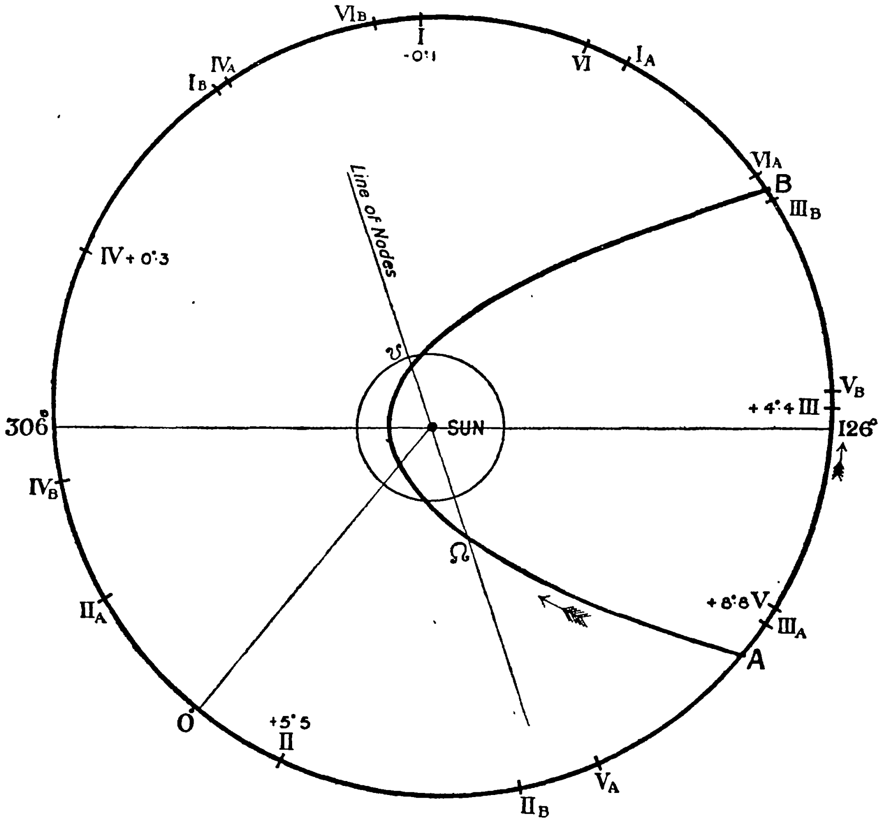
Year.	Eccentricity.	Perihelion Distance.
1531	0·9684	0·568
1607	0·9669	0·584
1682	0·9677	0·583
1759	0·9676	0·5845
1835	0·9674	0·5866
1910	0·9617	0·6872

The change in the distance is a full tenth of the Earth's distance from the Sun. I do not assert that such a change is impossible, but it is certainly desirable that it should be verified, as I strongly suspect that there is a confusion in the position of the decimal point, and that the actual change is only $\frac{1}{10}$ of that given.

I have prepared a diagram showing the portion of the comet's orbit inside the orbit of Jupiter. The numerals I., II., III., IV., V., VI., indicate the positions of Jupiter at the instant of the comet's perihelion passage in 1531, 1607, 1682, 1759, 1835, 1910, while the same numerals with suffixes A, B indicate Jupiter's positions when the comet passed the points A and B. The distances of A, B from Jupiter's orbit are 1·0, 1·7 respectively. The



Orbits of Jupiter, Earth, Halley's Comet, showing positions of Jupiter, 1531, 1607, 1682, 1759, 1835, 1910.

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numbers near the numerals I., II., etc., indicate the Jovian perturbation of the comet's mean anomaly in the revolution following that passage. It will at once be seen that the figure opposite V. is abnormally large, although the approach to Jupiter was less close than for passage III. This increases the suspicion that the perturbations for the present revolution may be too large; indeed, in the case of the perturbation in eccentricity the suspicion rises almost to a certainty.

I do not know whether Pontécoulant's calculations are still in existence; the article in *Comptes Rendus*, vol. lviii., does not mention where they are to be found. Assuming that they are still in existence, a careful re-examination of all the figures might suffice instead of an entirely independent computation of the perturbations. For there is no question whatever of Pontécoulant's ability, merely a suspicion of possible numerical slips. The inconsistency that I have adduced above is of itself sufficient to demand such a re-examination, before the results can be received with confidence. One can hardly imagine a greater loss of prestige to astronomy than that which would arise if there were a notable error in the prediction of this return of the comet, after the wonderful success achieved in 1759 and 1835.

Assuming that the date 1910 May is correct for the perihelion passage, I think the complete failure of the Ångström curve is not without a warning to us. We have here a curve which admirably fits 25 successive passages, and yet the first time it is used to predict a return it breaks down utterly, the error being almost 3 years, or three times the largest previous error. This is a decidedly startling fact, and indicates the danger of using terms of an empirical nature in lunar or planetary tables. Where such are used, it is at least desirable that means should be provided for readily effecting their removal from the calculated positions.

The Proper Motion of Castor. By A. C. D. Crommelin.

In the determinations that have hitherto been made of the proper motion of Castor, it does not appear that any effort has been made to separate true proper motion from orbital motion; the values given have merely been deduced from the meridian observations of the principal star, from the time of Bradley to the present day, on the assumption of uniform rectilinear motion. This assumption was fairly justifiable for the 130 years from 1755 to 1885, during which the curvature of the orbital path was slight; it is now rapidly increasing, so that the orbital motion in 1906 is at right angles to that in 1820, and in a few years the accepted proper motion will become entirely erroneous. There is as yet scarcely a long enough arc covered by the meridian observations to make a reliable determination of the relative masses from them. An attempt is made by Mr Lewis and Mr Furner in